

THE PSYCHOLOGICAL BULLETIN

UPRIGHT VISION AND THE INVERTED IMAGE.

BY DAVID COYLE,
Princeton University.

The eye-movement theory of upright vision has been taken by some writers as necessitating the inversion of the retinal image. Croom Robertson¹ says: "*If the image were not upside down we should not see the object the right way up.* For in looking up to the top of the pillar our object is to bring the image of the top on to the *lutea macula*, and as the eye is globular, the raising of the cornea in front depresses the *lutea macula* at the back. So that, in fact, if the rays did not cross as they do (thus inverting the image), the raising of the iris would bring the *lutea macula* opposite the rays from the bottom of the pillar." According to Wundt:² "If the position of objects in space is inferred from movement, the retinal image *must* be inverted, since only where this is the case is it possible for the movement to correspond with the actual position of the objects. So far from being a paradox, the inverted retinal image is necessary for vision. The retinal image must have been upside down, even if the laws of the refraction of light in the eye had not rendered the inversion physically necessary." And Stratton³ says: "Upper and lower, according to [the eye-movement] theory, mean positions which require an upward or downward movement of the eye to bring them into clear vision. But an upward movement of the eye brings into clear vision only what lies below the fovea on the retina. So that [on this theory] the perception of objects as upright requires that their retinal images be inverted."

It has been shown experimentally by Professor Stratton⁴ that in-

¹ *Elements of Psychology*, ed. by Rhys Davids; New York, 1896; p. 129; italics as in the original.

² *Lect. on Human and Animal Psychology*, Eng. trans.; London, 1894; p. 164.

³ *PSYCHOL. REVIEW*, 1896, III., 611.

⁴ *PSYCHOL. REVIEW*, III., 611 f.; IV., 341 f., 463 f.

version is not necessary for upright vision, and this fact has been used as an argument against the eye-movement theory.¹

Now it can be shown, by a consideration of the optical principle involved, that the assumption of the necessity of the inverted image is erroneous.

In the first place, let us consider a normal eye, with image inverted, as in Fig. 1. Now to bring the image of B on the fovea, f , the eye is turned upward till its axis passes through B , when the image

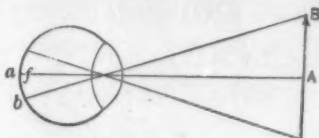


Fig. 1

of B will necessarily fall on f . Since we are not aware of the relative positions of f and b on the retina we do not purposely move f toward b , but by contraction of the proper eye muscles we move the eye so that we will perceive B more and more distinctly. The physical counterpart of this is that b and f are growing nearer each other, which is the necessary and sufficient condition of the impression of increasingly clear vision, *independent of what may happen to be the absolute motion of the eye.*

It only remains to prove that in the cases where b is above f a lifting of the eye will again make b and f approach, even though f actually moves downward. Consider the case of an eye equipped with a set of lenses, as in Fig. 2, which will cause the image on the retina to be upright. Let AB be an object, evidently its image will be in the position ab . Now aA coincides with the axis of the system, and it is evident, since A is the point whose image is at a , *i.e.*, at the fovea, that any point, to be focussed at the fovea, has only to be brought to the position A , or to some other position on the axis of the system. This gives us the following principle for the motion of an eye with the erect image: to bring the image of any point onto the fovea, it is necessary to turn the eye so that the given point is directly in front of the lens. That is, one must turn the eye upward to see high objects, and *vice versa* — exactly as with the inverted image — which is the necessary and sufficient condition of upright vision.

¹ Professor Stratton himself says his experiment "makes the eye-movement doctrine of visual directions of little practical assistance for understanding the harmony between sight and touch." *PSYCHOL. REV.*, IV., p. 481. Cf. his *Experimental Psychology*, p. 146.

Let us consider the case of an eye fitted so as to have an erect image, and whose retina has never been used, and has therefore no retinal local sign associations. Suppose it to be uncovered in a dark room with a point of light on the ceiling. A point on the upper part of the retina will be stimulated, giving the usual reaction in a tendency to movements of the eye. By trial the subject will find that raising the eye will cause the light point to approach and finally to

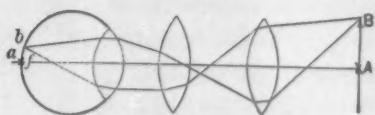


Fig. 2

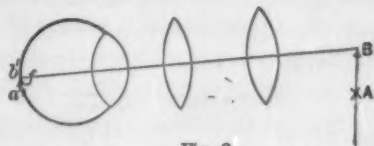


Fig. 3

reach the point of clearest vision. In Fig. 2, as the eye revolves upward the retina moves downward and the image of *B* moves downward to coincide with the fovea at *b'* as in Fig. 3. (For the effect of this motion on a retina *accustomed to motion in the opposite direction under these circumstances*, cf. the 'swinging of the field' experienced by Professor Stratton.) It will be seen from the above that a person born with an erect-image eye would have exactly similiar materials for the formation of local sign associations by movements of the eye as one with the inverted image. For, since the system of lenses is supposed rigidly connected with the eyeball, the reasoning holds good also for a system in which the lenses would be inside the eye itself.

PSYCHOLOGICAL LITERATURE.

COLOR-BLINDNESS.

Fortgesetzte Untersuchungen zur Symptomatologie und Diagnostik der angeborenen Störungen des Farbensinns. W. A. NAGEL. *Zeitschrift für Sinnesphysiologie*, 1906, XLI., 239-282, 319-337.

Nagel has been engaged, for upwards of ten years, in the investigation of color blindness; and besides his own researches, much work has been done under his direction by his colleagues at Berlin. A chief purpose of Nagel's endeavor has been to obtain a more accurate diagnosis of the abnormalities of color vision, — the group of phenomena which have come to be known as 'color weakness' receiving particular attention. The results of these many years of painstaking research are summarized in the present paper, which also contains a discussion of the theoretical and practical bearing of the data presented. The paper is a most valuable contribution to the literature of color vision.

The characteristics of 'color weakness' have been investigated in detail by Guttmann (*Report of the Giessen Congress, 1904*, p. 15) and by Nagel. A prominent feature of this defect is the uncertainty and confusion which arise when color stimuli are exposed with a small visual angle. Under this and other relatively unfavorable conditions of vision 'color weakness' is found to be identical with anomalous trichromatism (as defined by Koenig). Several thousand cases of 'color weakness' examined by the author contained not a single instance of the defect in pronounced form which did not turn out on closer examination to be anomalous trichromatism. 'Color weakness' is therefore to be regarded as a form of color blindness; it is not a mere reduction of normal color sensitivity, as has so often been supposed. Neither in the literature nor in the testing of color vision for practical purposes has the significance of this defect been recognized. The anomalous trichromate is no less a menace in the railway and marine service than is the dichromate. The navigator or the locomotive engineer is frequently obliged to interpret signals when they subtend small visual angles, when they are partially obscured by fog, and when they appear for but an instant. And these are just the optimal conditions for the confusion of colors on the part of the anoma-

lous trichromate. Nagel is convinced that many wrecks are traceable to this defect of color vision, and more primarily to the defective color testing of employees.

It was inevitable that methods of testing the color sense should be a question of prime importance in Nagel's investigation. And it is here that the author has done most valuable service. Nagel is himself possessed of an anomalous color system (deuteranopia), and is for that reason peculiarly fitted for the task of evaluating methods of testing. His experience with the tests hitherto employed convinces him of their utter unreliability, — a conviction which has been shared by numerous other investigators. But the author has gone farther: he has devised and perfected a simple and ingenuous form of test which seems to be free from objection. Indeed, it has won the approval of his colleagues in Germany, and has been adopted by the Prussian minister of railways and by the imperial naval authorities. It is the group of anomalous trichromates which has shown the inadequacy of the tests in current use, although it turns out that the current methods may even fail to detect the presence of dichromatism. Thus, of three hundred railway employees, all of whom had been passed by physicians, Nagel found five per cent. to be typical color blinds. And more surprising still is the result obtained in the examination of 1778 members of the railway regiments who had been detailed for an official determination of the utility of Nagel's method of testing the color sense. All of these men had passed the Holmgren test at least once, and the majority had also been pronounced to be normal by physicians who employed the Stilling test. Yet Nagel's method — employed, as in the case of Holmgren's and Stilling's, by military physicians — revealed the presence of thirteen dichromates and thirty-one anomalous color blinds of various types.

Nagel's test is put upon the market in the form of twenty pseudo-isochromatic plates with detailed instructions for their use. These colored plates are adapted to the detection of all the known forms of color blindness, — each form being revealed by a typical reaction to particular plates. It is of course impossible to give here an intelligible description of the test, nor would it be profitable to attempt such a task save in the presence of the plates themselves. The reviewer cannot too strongly recommend the adoption of this test by psychologists (W. A. Nagel. *Tafeln zur Untersuchung des Farbenunterscheidungsvermögens*. Vierte vermehrte Aufl. Wiesbaden, J. F. Bergmann, 1906.)

Erworbene Tritanopie. COLLIN und W. A. NAGEL. Zeitsch. f. Sinnesphysiol., 1906, XLI., 74-88.

Collin and Nagel describe three interesting cases of acquired abnormality of color vision, two of which they classify as tritanopia (von Kries) or violet-blindness (Helmholtz). The first patient was a student who had received a 'Durchzieher' in the region of the left eye. The eye-lid had been incised and the orbital wall fractured, but the eye-ball itself had received no external injury. Normal color vision failed to return when the wound was healed, hence the case was submitted to a thorough investigation. Both eyes were found to be emmetropic, and the right was normal in appearance and in function. In the fundus of the left eye there was found a small gray patch in the region of the macula, and in the extreme peripheral region a blood clot, several ruptures of the retinal blood-vessels, and an oedematous condition of the retina, — all due, it is believed, to the wound and fracture. The color sense of this eye was strikingly abnormal, a condition which gradually disappeared with the disappearance of the abnormal condition of the retina. In the early stages of the test the patient reported that yellow-green appeared to be blue, and that yellow (Na-line) seemed pale violet (lilac), when the stimulus color subtended a visual angle of 1.5 degrees or less. These colors were seen normally by the right eye, and binocular regard gave retinal rivalry (yellow and violet). Red was seen normally by the injured eye, excepting that it appeared more saturated than to the normal eye. At about 575-580 $\mu\mu$ the violet (yellow) passed abruptly into blue (green); at this region too was found a narrow band of almost uncolored light. Bluish began at 560 $\mu\mu$, and 'distinctly blue' at 545 $\mu\mu$ (yellow-green to the normal eye); 430 $\mu\mu$ was reported to be dark blue (distinctly violet and brighter to the normal eye). The spectrum was of normal length at the red end, but was very much shortened at the violet end. Two weeks later the pathological condition of the macula was no longer visible; nor was any abnormality of color vision revealed in tests with the long-waved half of the spectrum. But blue (470 $\mu\mu$) seemed greenish blue, and violet seemed blue; the latter stimulus however assumed a violetish tinge with long-continued regard.

The second patient suffered from albumino-uric retinitis with complications. In the early stages of the tests this patient confused yellow and green with gray, but she had no difficulty with red or blue. Nine days later it was found that her previous equations of yellow and green no longer held. Yellowish green in juxtaposition with blue seemed gray; when yellowish green was exposed beside yellow, the former was

called blue and the latter white. Upon a white back-ground yellow and green both seemed blue. Tests made twenty-four days later showed a marked decrease of abnormality of the color system.

The third patient was a workman who suffered from severe headache. The ophthalmoscope showed a temporal bleaching in both eyes; otherwise the fundus was normal. This patient confused blue with green, and yellow with green. Red was invariably recognized. The Nagel plates made up of spots of yellow-green and blue-green were reported to be of a uniform green color. In tests with spectral colors (visual angle of 2° - 3°), yellow green ($570\mu\mu$ - $575\mu\mu$) was called white, gray or yellow; violet ($430\mu\mu$ to end of spectrum) had no distinctive color. The paper closes with a discussion of the results obtained in these tests.

Ein Fall von Grünblindheit (Deuteranopie) mit ungewöhnlichen Komplikationen. ALFRED GUTTMANN. Zeitsch. f. Sinnesphysiol., 1906, XLI., 45-53.

This case had been tested repeatedly 'by different methods' and pronounced normal. The Nagel test however showed the presence of an abnormal color system, and a thorough investigation was undertaken. The Helmholtz color-mixing apparatus was arranged to show a semi-circle of red ($670\mu\mu$), and a semi-circle of yellow ($590\mu\mu$); the patient reported that he saw a uniformly colored yellow disc. (It is not clear why this case should be called *Deuteranopia* rather than *Protanopia*.) A somewhat similar confusion seems to have been obtained with red and green light ($670\mu\mu$ and $535\mu\mu$). In tests with pigments, a mixture of 8° blue and 352° black-white proved to be indistinguishable from a mixture of 49° of the same blue and 311° of black-white. Nor could the patient distinguish between mixtures of 12° blue + 348° black-white and 50° yellow + 310° black-white. Yet he was not blue-blind; it would appear from the author's statement that blue and yellow stimuli were confused only when presented in relatively slight degrees of saturation. Another striking feature of the abnormality was the sub-normal sensitivity to differences of brightness in colored and uncolored stimuli. Patches of spectral red ($670\mu\mu$) were exposed, and while one remained constant in brightness the other was varied until the judgment 'brighter' or 'darker' was reached. It was found that the width of the slit could be varied between limits represented by the units 20 and 40 without any perceptible change of brightness; for the normal subject the corresponding readings were 25 and 26. Yellow ($589\mu\mu$) gave limits of 17-31 for the abnormal and

19-21 for the normal subject; green (535 $\mu\mu$) 19-35 for the abnormal and 22-24 for the normal; blue (460 $\mu\mu$) 40-111 for the abnormal and 61-63 for the normal. This case, then, is characterized by a lack of red-green sensation, and by a reduced sensitivity to yellow-blue and to brightness.

J. W. BAIRD.

UNIVERSITY OF ILLINOIS.

FUNCTION OF THE EAR.

Die akustische Function der lufthaltenden Hohlräume des Ohres.

F. KRETSCHMANN. Archiv f. d. ges. Physiologie (Pflüger's), 1905, CVIII., 499-536.

The extensive system of pneumatic cavities formed by the tympanum, the mastoid antrum and the numerous cells about the bony labyrinth constitute a resonance-chamber by means of which sounds otherwise too feeble to be detected are rendered audible. This is demonstrated by an ingenious series of experiments with resonators, auscultation tubes and specially prepared acoustical apparatus. The importance of the spongy structure of the bony walls is shown by filling a resonator with bits of porous coke which damp the fundamental tone of the resonator without decreasing the response to sounds of other pitch. Change of tension of the tympanic membrane is shown to aid in damping fundamental tones, in protecting the inner ear against violent sounds and in accommodating for very weak sounds and for extremes of pitch. The superiority of the bow-shaped arrangement of the chain of ossicles, as compared with the straight columella of the bird-ear, consists in making possible this change of tension. Sound vibrations are transmitted to the labyrinth, not only by way of the ossicles and the oval window, but also through the round window and through the bony capsule. These auxiliary paths could not function efficiently without the system of pneumatic cavities.

The reviewer has been led to question this last conclusion through some tests which have recently been made in the Chicago laboratory upon a subject whose hearing is fairly good, despite the fact that the drum membrane and the ossicles have been removed from both ears. Dr. Kretschmann's description of the function of the chain of ossicles seems to be wholly admirable, as well as his insistence upon the importance of the two secondary conduction paths, which receive scant treatment in the ordinary accounts of hearing.

W. V. D. BINGHAM.

UNIVERSITY OF CHICAGO.

ATTENTION.

Physiological Factors of the Attention Process. W. McDougall. Mind, 1906, XV., 329-359.

Ueber die Intensitätsänderungen schwacher Geräusche. W. HEINRICH. Zeitschr. f. Sinnesphysiol., 1906, XLI., 57-58.

Ueber das periodische Verschwinden kleiner Punkte. W. HEINRICH und L. CHWISTEK. Zeitschr. f. Sinnesphysiol., 1906, XLI., 59-73.

The paper by McDougall is the last of a series, the three preceding numbers of which appeared in the same journal in July, 1902, and July and October, 1903. In this paper McDougall supports the theory that the important conditions of maintenance and change of attention are cerebral, and the part played by motor adjustment is secondary. Three types of condition are noted: (1) *External*, including intensity, novelty, suddenness, contrast, etc., of the stimulus. (2) *Accessory internal*, including amount of free nervous energy in the brain, which depends on the afferent currents, principally the visceral; and adjustment of sense organs, which directly favors stimulus reception as well as adds to the afferent current; these are rather effects of attention direction which support and maintain it. (3) *Intrinsic* conditions, *i. e.*, the varying states of the higher-level systems.

The factors determining the intrinsic conditions, *i. e.*, deciding which higher-levels shall function at any time, are detailed: (a) Excitement of the system by expectation or concomitant reinforcement, as in voluntary attention (volition in general), or by residual activity, or by afferent nerve currents set up by discharges to the viscera by the same higher system; a circular process (emotional interest). (b) Inhibition of other systems by the one functioning. For this McDougall adopts James' 'drainage' theory as being simplest, since it also explains association. (c) Fatigue of the systems, leading to temporary lapse of activity and allowing another system to become active. Fluctuation of attention in general is ascribed to cerebral fatigue. Even in the case of minimal stimulations the sense-organ changes are results rather than causes.

The vascular changes McDougall considers of no importance, since any one system of neurones is supplied by many parts of the vascular system and, conversely, each vascular system supplies many neurone systems.

The experimental evidence adduced by McDougall is principally

derived from reversible spatial percepts and retinal rivalry. As against the importance of motor adjustment, he cites the changes in perspective with unipunctual fixation and attention, and with accommodation changes contrary to the normal direction; the grouping of double-rowing white dots in percept or after-image in rows either with or contrary to eye movements, or with constant fixation; and the voluntary lengthening of either phase in any fluctuating percept or image.

The importance of the *intrinsic* factor is shown (1) by constancy of reversible figure until another position is suggested to observer; (2) by voluntary control of fluctuations; and (3) by the fact that after-images appear in whichever phase you think of at the moment of appearance. In case of white dots, the after-images may appear irregularly unless voluntarily grouped in rows, in which case they appear and disappear by rows. With red and blue groups of dots viewed stereoscopically, either field may be retained longer by attending to alternate groupings in that color. In this connection three interesting cases of alleged cerebral induction of light and color are detailed. These have, however, only indirect bearing on the theme.

The effects of fatigue. Unusual length of one phase of an alternation percept, produced by voluntary effort or by mechanical means (as closing one eye or introducing movement in one field in retinal rivalry, or introducing binocular factors in the 'windmill' illusion) is always followed by unusual length of the opposite phase when the inhibiting conditions are removed.

At the conclusion, McDougall admits that a thoroughgoing physical explanation does not really satisfy him. In all attention there is a convergence of the energy from all or many parts of the brain into one neural system. In voluntary attention there is still further concentration, and at this point, he thinks, there may be a 'psychical guidance of physical processes.'

Heinrich offers an explanation for the fact, alleged by him in earlier papers, that some observers fail to detect fluctuations in minimal tones or clangs when these are free from 'noise.' In the case of tones or clangs which contained noise, Heinrich reports in the present paper that the one subject found the tones to remain constant, and the noise to vary.

The tympanic membrane, according to Heinrich, requires especially fine tuning for weak noises. Reaction of the membrane to tones may be observed even when the tension is slightly changed from the proper correspondant to the tone, but with noise the membrane is astonishingly

indifferent unless exactly tuned. The fluctuations of minimal noises is therefore explained as due to periodic variations in the tension of the tensor tympani, which do not affect the perception of tones. This theory seems premature, in view of the fact that there are many possible reasons why some may fail to observe fluctuations in 'pure' tones while others do observe them.

Hammer's explanation of fluctuation as due to objective causes Heinrich offsets by experiments on four persons with the same clock. The tests apparently were not simultaneous. He finds that the fluctuation period is different for each person, and is variable for each, whereas he assumes that if the fluctuation were due to physical variations in the sound of the clock they would be regular and the same for each person, since the revolutions of the wheels are regularly repeated. This assumption seems to the reviewer to be rather uncritical, not taking account of the fact that the irregularities, if such exist, are due not to irregularities of *one* wheel, but of several wheels revolving at different rates, so that the cycle of the relatively pronounced irregularities is apt to be highly complicated and may take several hours to complete itself. The experiment must therefore be regarded as irrelevant.

The experiments of Heinrich and Chwistek are based on an earlier statement of Heinrich's that the disappearance of small, sharply fixated points is due to fluctuations in the crystalline lens. In the first experiments the patients, two in number, and with normal vision, fixated black points on light ground, recording (on kymograph) appearance and disappearance by pressure on a rubber bulb. The experimenter, viewing through an ophthalmometer the images of a light reflected to the lens of the patient's eye by two mirrors, was able to note slight changes in curvature of the lens, and record them by another bulb, along with the patient's record. It was found that the disappearance of the point in most cases coincided with a change in the lens. In a few cases there were changes without disappearance, and *vice versa*. There was no question of fatigue, for the changes in the lens commenced with the fixation.

In the second and fourth experiments upon three subjects, with different means of registration, and chronometer record, it was found that with increasing size of the black spots there was increasing length of the fluctuation period, and relative lengthening of the period of visibility. This is in accord with the assumption that the larger variations in the lens are the less frequent. It was also found that with darkening of the background there was shortening of the fluctuation period, with relative shortening of the phase of visibility. Tables

showing averages and mean variations are given for both these experiments, but the number of tests on which the averages are based is not stated. Optical diagrams showing how lens-changes could produce the fluctuations are also given. Two of these patients were myopic. The third experiment, made on these, showed that with two adjacent spots beyond the range of clear vision there was a periodic increase in distinctness, shown by the separation of the two spots which otherwise fused into one. The authors very properly do not claim that these experiments demonstrate the dependence of the fluctuations on changes in the lens, but only that the two are synchronous processes.

KNIGHT DUNLAP.

JOHNS HOPKINS UNIVERSITY.

Untersuchungen über das periphere Sehen: Ein Beitrag zur Psychologie der Aufmerksamkeit. STANISLAW LORIA. *Zeitsch. f. Psychol. u. Physiol. d. Sinn.*, 1905, XL., 160-186.

In an investigation of vision with instantaneous illumination, Helmholtz (*Physiol. Optik*, 1866, 471 f.) found that at the instant when the spark appears, the observer obtains a distinct impression of only that part of the field towards which the attention is for the moment directed. He reported too that, while preserving a constant fixation, one may direct one's attention at will to any region of the paraxial field. These and similar results of that period supported the assumption that the attention is a purely central function whose direction is independent of any motor mechanism. And numerous writers have accepted the principle that a mere effort of will—without the coöperation of any motor adjustment whatever—suffices to bring to the focus of consciousness whatever part of the content one may choose to select for attentive examination. Professor Heinrich's investigation of the phenomena of attention led him to dispute the validity of this principle. This author showed that while it is unquestionably true that different regions of the visual field may be attended to without change of fixation, yet these shiftings of attention are accompanied by variations in lenticular adjustment (*Zeitschrift*, IX. and XI.). Heinrich's measurements, however, were confined to certain circumscribed regions of the lateral field of vision, and, too, his investigation failed to determine whether paraxial accommodation is accurate or approximate only. Hence Loria has been led, at the instigation of Heinrich himself, to pursue the investigation farther.

Loria undertakes to determine what is the relative influence of the distance of the fixation-point, and of the position of the lateral object, in initiating an adjustment of accommodation when the attention is

turned to the paraxial object; he undertakes also to measure the range of paraxial accommodation for constant positions of the fixation-point, *i. e.*, to determine the locations of the near-points and the far-points of accommodation upon various radii of the visual field. His apparatus was a perimeter whose fixation-point and stimulus object were adjustable in distance from the eye. And his method consisted essentially in determining, for different radii of the horizontal plane of the visual field, the limits within which the distinct vision of a minute lateral object is possible without variation of fixation. Thus, with the fixation-point at a constant distance, the momentary far-point and near-point of paraxial vision were determined upon radii representing degrees of excentricity varying from 10° to 50° . These measurements were made for various degrees of contraction of the ciliary muscle in the atropinized and in the normal eye. Loria's results confirm and supplement the findings of Heinrich. The lenticular adjustment changes when the attention is directed from the axial to the paraxial regions of the field of vision — notwithstanding the fact that the fixation upon an axial point remains constant throughout. The magnitude of this accommodative change varies with the variation in the excentric direction of attention; the line of paraxial accommodation not only shortens, but its location progressively approaches the eye in proportion as more lateral regions of the field are reached — the normal eye being myopic in excentric vision. And in indirect vision the lenticular adjustment is determined, not by the distance of the fixation-point, but by the position of the paraxial object of attention — the region within which the paraxial object is seen distinctly being wholly independent of the distance of the fixation-point from the eye. Thus, for an emmetropic eye, the distance of the momentary far-point varies progressively from 100 cm., when the paraxial object is 10° from the visual axis, to 20 cm., when the object is 50° from the visual axis; the shifting in the position of the near-point is inconsiderable. The exploration of a certain radius (30° from the visual axis) of the visual field shows the region of clearest vision to be 12 cm. from the eye when the fixation-point is 120 cm. distant, but when the fixation-point is brought in to 65 cm., the region of most distinct vision remains practically unchanged, at 11 cm. Loria describes an interesting experiment which deals with the simultaneous vision of various paraxial objects. He finds that when a number of objects are exposed simultaneously at different momentary points of clearest paraxial vision, they are seen simultaneously in maximal clearness. The paper closes with a discussion of the bearing of his findings upon theories of attention.

UNIVERSITY OF ILLINOIS.

J. W. BAIRD.

MEMORY AND ASSOCIATION.

Merkfähigkeit, Gedächtniss und Assoziation. Ein Beitrag zur Psychologie des Gedächtnisses auf Grund von Untersuchungen Schwachsinniger. KURT GOLDSTEIN. Zeitschr. f. Psych., 1906, XLI., 38-47, 117-144.

Convinced that a consideration of pathological changes in intellectual capacity, such as those exhibited in imbecility, may be of value to normal psychology, our author has made a number of psychological experiments upon patients suffering from various forms of weak-mindedness. He declares his investigations to differ from those of the Kraepelin school in that they relate, not so much to single portions of the psychical personality, as to a comparison between single primary defects and the remaining psychical status. His present report is limited to the results of certain investigations concerning memory and its relation to association.

The experiments consisted in a series of tests of (1) memory and (2) associative ability. Under (1) the capacity for retaining new impressions is distinguished from the virtual memory test of reproducing knowledge acquired in youth. The effect of distracting stimuli introduced during the interval between the impression and the reproduction was also tested. Under (2) tests were made of associations already at the patient's command; also tests of capacity for acquiring new associations. The patients were all women, and imbeciles; three congenital, one paralytic, one epileptic and two senile.

The results lead the author to distinguish between memory in stricter sense, and capacity for noting (*Merkfähigkeit*). Though both involve the processes of simple impression and association between the things noted, yet the two are not at all made use of in equal measure. Real memorizing depends upon association, whereas a mere capacity to note depends most largely upon impressibility (*Einprägungsfähigkeit*). When a considerable time interval separates the noting and the test by reproduction, association tends to aid the reproduction. But when the interval is short, it rather hinders than helps. The shorter the interval, the more important is the simple impression, and the more distracting the intervention of associations. The longer the interval, the less important is impressibility as an aid to associative activity.

The capacity for associating things noted belongs to the general mechanism of association. Impressibility, however, is independent

therefrom. Consequently the two may appear in contrary degrees in the same individual. Congenital imbecility is characterized by a well developed impressibility, but marked deficiency in powers of association. Acquired imbecility is characterized by a tolerable capacity of association, but slight impressibility. The latter class may retain knowledge fairly well, although the capacity for noting is minimal. With the former class capacity for noting may be quite good with respect to retention for brief intervals, although the matter is never fully understood.

The acquisition of knowledge depends upon both impressibility and a capacity for associating things noted. Still, these capacities do not necessarily lead to the acquisition of knowledge. In such acquisition apperception plays an important part. The chief defect of congenital imbecility may, therefore, be referred to deficiency of apperceptive ability as conditioning the incapacity of association.

ROBERT MORRIS OGDEN.

UNIVERSITY OF TENNESSEE.

Immediate Memory in School Children. W. H. WINCH. British J. of Psychol., 1906, II., 53.

The author has performed a series of experiments with the following questions in view: "1. Whether pure memory, that is memory of percepts associated only in time and space, is improvable by practice. 2. Whether such memory tends to improve as children rise in age and standard in school studies. 3. Whether such memory has any relation to general intellectual proficiency as measured by the position of children within their classes."

The experiments were performed on students selected from the classes of a London girls' school, by the following method: Sets of consonants, twelve for each exercise, were called out, each series being twice given and the two repetitions occupying 25 seconds. The children were allowed one minute 35 seconds to reproduce this in writing. Three such series were given with intervening periods of exactly a week. After a month's interval, necessitated by the Christmas holidays, a fourth set was given, and exactly a week later the final list.

The results show: "1. A marked and almost invariable improvement, slightly interrupted in some cases by the interval of holiday. 2. School proficiency, as measured by age and standard and position in school, appears to be generally accompanied by good memory of the kind we are here testing, though not invariably so. 3. That when the comparison is narrowed down to children of the same grade or

standard, it is still found that memory is positively related to position in school, though, as we should expect, with less exactitude than when a wider range of age and standard of school proficiency is taken for comparison."

The author adds a table of numerical results, showing the comparison between memory and general proficiency.

Rückwirkung sprachlicher Perseveration auf den Associationsvorgang. A. PICK. Zeitschrift für Psychologie, 1906, XLII., 241-257.

The author proposes to investigate the certainty and extent of reaction of the persistence of the vocal motor activity involved in speaking certain words, upon the associative activities of the mind.

After calling attention to the well known influence of bodily position upon the contents of consciousness in pathological and child psychology, and observing that such phenomena are not unknown to normal adult psychology, he proceeds to give full accounts of his observations and experiments upon two pathological cases, the one an epileptic of thirty years' standing, the other a man recovering from convulsions.

Both cases when examined were in a state of dim half-consciousness in which they were abnormally sensitive to suggestion. In the case of the epileptic woman the ideas *match* and *candle* were suggested until they were firmly fixed in her mind. A piece of bread was then given her, and she was asked what it was. She replied 'a match,' and then persisted in using it as a match. When a crumb of it was put into her mouth she rejected it with signs of nausea. A cup which was given to her without her naming it was treated at first as a match but immediately afterward put to its proper use.

That an objectively false presentation is rendered decidedly more persistent by the utterance of the name of the object for which the one presented is mistaken, is the conclusion drawn from a long series of similar tests applied to both subjects. In the case of the man recovering from convulsions a mixture of the idea of the thing vocally named with that of the actual object presented and with other ideas vividly present in the mind was observed. This was shown by a resulting confusion both in speech and action, but the preponderating factor was always the idea associated with the words spoken by the subject.

The author concludes that, since all the factors of pathological psychology differ from those of the normal in relative intensity and

prominence alone, and not in their intrinsic nature, the question of the influence of the persistence of speech-activity as a factor in normal psychology is one which deserves attention. The author is to continue the subject in a later issue.

S. VIDA ROSS.

UNIVERSITY OF CALIFORNIA.

Studien über die experimentelle Beeinflussung des Vorstellungsverlaufs. MAX LEVY. Part I. *Zeitschrift für Psychologie*, 1906, XLII., 128-161.

Dr. Max Levy presents in this article a study in association based on observations made upon patients in an insane asylum, making use of the well known fact that in several forms of insanity patients react to a word they may hear — even though it may not be addressed to them. Spontaneously they pour forth a stream of utterances for which the given word seems to have been the stimulus.

Before giving his results Dr. Levy criticizes the common method of experiments on association. (1) When a subject is told to speak out whatever words occur to him in response to a given one that serves as a stimulus, he is placed in a situation utterly unknown in his normal life. We are trained and accustomed to make a selection between the different possibilities of reaction and not to follow any association that may arise without consideration. (2) Besides we are not accustomed to react to an isolated word without any kind of purpose in view. Hence it would appear that the normal flow of thought is very different from that secured in the experiments on association.

In the classification of associations writers have been somewhat arbitrary — following logical divisions rather than psychological facts. For example, one cannot tell from two words alone whether or not the association between them is based upon their meaning or the custom of having seen two things always together.

The author calls attention also to the different effect that one and the same word may have when spoken to the same person on different occasions or to different people.

The first part of the study is brought to a close with a consideration of what the author, following Ziehen, terms the 'constellation' of an idea. This is a factor that helps to bring an idea into consciousness as well as the three other factors of its 'associated relationship,' its 'tone of feeling' and its 'clearness.' The 'constellation' of an idea is its relation to all the ideas and feelings with which it has ever been connected. It embraces the influences of environment, the circum-

stances of childhood, of our occupation, etc. The attempt is then made to prove the influence of the 'constellation' by a rather limited number of experiments on insane patients.

THOMAS V. MOORE.

UNIVERSITY OF CALIFORNIA.

Experimentell-psychologische Untersuchungen über das Denken.

AUGUST MESSER. Arch. f. d. ges. Psychol., 1906, VIII., 1-224.

A very full and thorough investigation into the nature and process of thought is presented by Herr Messer in his experimental research. In addition to original interpretation, he gives us the statements of the subjects themselves wherever necessary, besides acknowledging the work of Watt, Münsterberg, Binet, Lipps, Meinong, and a number of others, psychologists and logicians, who have worked in this field.

The author's method of procedure was as follows: Fourteen series of tests were given. In the first, the subject was given a printed word visually, upon the understanding of which he was asked to give the first word arising. The stimuli were 143 one and two syllabled substantives, representing objects of a concrete kind. In the second series the subject was asked to name a coördinated object. In the third series he was asked to give a coördinated concept. Seventy-one words of series one were used in series two and three. In the fourth series, the subject was asked to name any adjective; in the fifth, a characteristic of the concept represented by the word; in the sixth, an object symbolized by a subordinate concept. In series 7-11 the stimuli were either paired words or four or five syllabled words. In the seventh, the subject was asked to give the relation between the concepts represented by the words paired; in the eighth, the relation between the objects symbolized by the words. In the ninth series, the stimuli were paired familiar proper names (of philosophers, artists, statesmen, etc.), which were to be compared, the object of this test being to get a judgment of objective value. In the tenth, similar names of persons, things, etc., were given in pairs, of which the subject was to judge which was preferable, the object being to get a judgment of subjective value. In the eleventh series, names and adjectives were paired. The subject was requested to apprehend them either as assertions or as interrogations, to take an attitude towards them and if possible to pass a judgment on them. In the twelfth series, a number of sentences or word groups were suddenly presented, upon which the subject was to take an attitude towards them, and to understand the meaning involved. In the thirteenth series, objects or pictures were

presented and the subject was asked to give the words arising. In the fourteenth, the subject was asked to make some statement concerning these objects and pictures. Reaction times were taken, and full introspection was expressed by the subjects.

In all these tests, one good feature was this: The subject stated freely what he actually experienced in the processes undergone in the various tests. 'What did you experience? What experience did you undergo?' was emphasized in all the series. And these full statements as given by the subjects are reproduced by Herr Messer in full throughout the pages of his most excellent work. I shall make no attempt to give all the results, or even any small part of them. I shall try simply to indicate the more important features brought out by the author.

Herr Messer finds that of the concomitant images, the visual predominate as guiding agencies, though they are of little account in the actual apprehension of meaning. These visual images seem to rise spontaneously and call for little comment on the part of the subjects. Where an image is at first dim, in the process of explaining the process involved in the test, such image becomes more clear and distinct. Concerning the motor concomitants, it seems that the subjects often were unable to say whether the experience was an actual motor idea, or simply a motor impression. But motor elements were decidedly present.

The connection between the associated words seems to be one of meaning, though at times association by sound seems to have operated. In the consciousness of the meaning of the words, there seem to be two forms of apprehension. In the one there is simply an unanalysable understanding of the words, in the other there is a more determinate apprehension of the meaning. In the latter case we have a certain, specific consciousness of particular valuation, more or less definitely localized. In no single instance does the author find that visual images aided in the apprehension of meaning, as such, though such images were of use in guidance.

Where a word seemed to have several meanings, Herr Messer found the following possibilities:

1. There seems to be a consciousness of meaning in general.
2. There seems to be a consciousness of ambiguity.
3. Sometimes a less common meaning arose, while the more common meaning was passed by.
4. Where two meanings are apprehended, they may be:
 - A. Concomitant.
 - a. Both being present, with one more to the background, the other more dominant.

b. With a conflict between the two resulting in a kind of fluctuation.

B. Successive.

5. Three or more meanings may be cognized either simultaneously or successively.

On the basis of the results of the various tests, Herr Messer classifies judgments from a psychological point of view as follows :

I. According to content.

- (1) Affirmative and negative.
- (2) Analytic and synthetic.
- (3) General and particular.

II. According to their relations to other judgments.

- (1) New or reproduced.
- (2) Complete or abridged (copula omitted).
- (3) Immediate or mediate.

III. According to the manner of presentation.

- (1) Perceptual or ideational.

IV. According to their relations to the subject judging.

- (1) Theoretical or practical.
- (2) Original or imitated.
- (3) Certain or uncertain.

The author then proceeds to characterize particular and general thinking. The former is marked by a certain external directness, a consciousness of reality, a concomitant visual idea or image, and a limitation to some specific concrete situation. The latter is marked by a lack of definite external determination, a lack of persistent visual imagery, by the predominance of the content and meaning of the representative words, by the increased importance of the word, and by a simpler, more gliding and mechanical process.

In addition to such aspects as relate to thought proper, the author also discusses psychic causality, the psychology of volition, and the psychology of individual differences. Altogether, the investigation is a remarkable piece of work. Even should the reader disagree with some of the conclusions, the complete data are there before him for his own interpretation.

FELIX ARNOLD.

NEW YORK CITY.

ILLUSIONS.

Ueber einige geometrisch-optische Täuschungen. F. KIESOW.
Arch. f. d. ges. Psych., 1905, VI., 289-305.

Ein Beitrag zur Kenntnis der variablen geometrisch-optischen Streckentäuschungen. LUIGI BOTTI. Ibid., 306-315.

The general point of these two papers lies in the renewed advocacy of the eye-movement theory of geometrical optical illusions and in the

presentation of fresh evidence in support of the theory. Both of the papers are confessedly preliminary, and both fact and theory are presented with scant regard for similar investigations in the same field. The figures examined are in all cases simple linear magnitudes. In the first paper the procedure is uniformly this. The standard line is a free horizontal from 15 mm. to 25 mm. in length. This is brought into comparison with an objectively equal line furnished with simple accessories which serve either to bound it or to form motives for illusory effects. No exact quantitative determinations of the illusory changes had been made by either observer at the time of writing. Hence the reader may be allowed a large measure of hesitation in accepting at their full value the interpretations placed upon the observations as they are now reported.

Two illustrations will serve to show the type of illusion examined in the first paper. In one case the illusion motive is provided by bounding verticals placed at the left end of the horizontal. These extend equally above and below the line, and run through a considerable range of length from a mere bounding mark to a line nearly five times the length of the standard. In the second case the illusion motive is furnished by a greater or less continuation to the left of the horizontal itself, the length of the latter being indicated by a short vertical mark through the line. In both of the cases named the free standard horizontal lies at the right, and under the two sets of conditions here stated the free horizontal appears invariably *shorter* than the objectively equal line that suffers an illusory change.

Three points are particularly emphasized in this paper. (1) Whatever influences the ease and rapidity of eye-movements, whatever, that is, offers incentives or checks to such movements, or supplies points of rest, is bound to affect the perceived length of a horizontal. (2) The vacillating character of many illusions, involving frequently an inability to pass definite judgment upon compared magnitudes, springs from purely optical conditions introduced by the very process of examination itself. (3) The influence of 'contrast' must not be applied in as simple a fashion as has often been customary. 'Contrast' may be a frequent coöperating factor, but its effects are often overshadowed by the eye-movement influences. In support of this claim there is an interesting analysis of the well known contrast figure of Müller-Lyer.

Under Kiesow's direction Dr. Botti continued the general examination of horizontals and comes everywhere to the same conclusions. He discusses particularly the influence of the position of a division

mark upon the perceived length of a line. The outcome is that while the placing of this mark at the middle of the line causes an apparent shortening, moving it towards either end produces at once an apparent increase of length. Dr. Botti also finds that a rectangle with a base approximately four times the altitude and formed of horizontal parallels has a maximum apparent height when the horizontals are neither too numerous to attract attention to themselves individually nor too few to produce a marked retardation of the vertically moving eye. Thus a rectangular figure with a base of about 55 mm. and an altitude of about 15 mm. seems highest when formed of six equidistant parallels, and much diminished in height when this number is reduced to four. The only possible interpretation seems to the author to be in terms of the degree of difficulty with which the eyes range over the vertical extent of the figure.

Though all real criticism of these papers should be withheld until their promised continuations have been given us, attention may at least be called to one point. Is it not hazardous and to a large degree profitless to talk glibly of the influence of eye-movements in the absence of any exact knowledge of what these eye-movements actually are in any given case? In this day of registration possibilities the cautious psychologist is not likely to find any satisfaction in the unverified assertion that the eyes are behaving thus and so. And until some graphic evidence can be furnished in support of such assertions, they must be viewed merely as hypotheses displaying cleverness but not carrying conviction.

Theorie der geometrisch-optischen Gestalttäuschungen. LUDWIG BURMESTER. *Ztsch. f. Psychol.*, 1906, XLI., 321-348.

This paper is concerned with certain simple illusions of reversible perspective. The title is misleading inasmuch as the theory sought is not psychological in character but of a strictly optical nature cast in terms of linear perspective. The object chosen for observation was a rectangular piece of cardboard, 100 mm. long by 30 mm. wide, mounted in weather-vane fashion upon a vertical rod which could be rotated by appropriate appliances. The head of the observer was fixed, and the cardboard rectangle and its reversed perspective were viewed against a background of gray. The point of the investigation was to discover the perspective principles which apply to the relations between the normal and reversed figures both when at rest and when moving about the axis of rotation. The results do not permit of convenient summary. And indeed the entire investigation seems to the reviewer to have only

a remote psychological bearing, its appeal being rather to the interests of perspective analysis. It is significant, however, to have a careful demonstration of the fact that the form of an illusory perception and its behavior towards the normal object can be treated from the point of view of mathematical optics.

A. H. PIERCE.

SMITH COLLEGE.

A Study of some of the Correlations of the Müller-Lyer Visual Illusions and Allied Phenomena. W. G. SMITH. Brit. J. of Psychol., 1906, II., 16-51.

The purpose of this study was to throw light on the meaning and relations of the Müller-Lyer illusions. The diminishing and the increasing forms of the illusion and a plain line, all of the same length, were estimated and reproduced by the observers in two ways, by visual perception and by kinæsthetic perception.

A correlation was discovered between the normal estimation of the plain line and the two forms of the illusion. The magnifying illusion was found to be greatest with the group of observers which underestimated the plain line, and least in the group in which it was over-estimated. The correlation with respect to the diminishing form of the illusion and the estimation of the plain line appeared to be direct in one group, while no correlation was apparent in the other. Smith accordingly formulates the principle that each form of the illusion tends to reach its maximum effectiveness where the preëxisting tendency in estimating the plain line is in the opposite direction. There is no correlation between the two forms of the illusion. An individual highly susceptible to one form is not necessarily equally susceptible to the other form. Accuracy in the estimation of the plain line is accompanied by lessened subjection to the illusion. Familiarity and practice due to the successive estimations had no marked influence in diminishing the effectiveness of the illusion.

ALEXANDER ROBBIE.

UNIVERSITY OF IOWA.

Ueber die Beziehungen von Zeitschätzung und Bewegungsempfindung. ERICH JAENSCH. Zeitsch. für Psychol., 1906, XLI., 257-279.

There is a somewhat familiar illusion of distance which manifests itself when one attempts to move, *e. g.*, the hand through two equal distances in the same straight line, in the fact that the movement is less when the muscles involved are already contracted. Jaensch

attempts experimentally to determine the factors upon which this depends.

The persons investigated were all, to a greater or less extent, subject to the illusion, the second movement being shorter than the first; and the time record showed an approximately equal duration for each movement.

In an analysis of the factors involved, the author rejects the comparison of intensities of the two motor sensations, because of an assumed qualitative change occurring with the progress of the movement. The visual factor is eliminated by having the eyes of the observer closed; and 'position sensation' on account of the same positions in the joints not recurring in the two movements. The speed factor is eliminated by having the movements made as freely as possible. His conclusion is, that if the judgment of distance in the two cases does not depend upon the above mentioned direct factors, it must depend upon an associative one. This he finds in the implicit estimation of the time duration of the two movements. They are regarded as equal because the duration of each is the same.

A further question as to why the second movement is more slowly executed than the first, leads to the conclusion that there is also a physiological factor involved, viz., the increasing muscular contraction.

F. S. WRINCH.

UNIVERSITY OF CALIFORNIA.

DREAMS.

L'évolution du rêve pendant le réveil. M. FOUCAULT. *Revue philos.*, 1904, LVIII., 458-481.

In this article, Foucault attempts to show that during the process of awaking from slumber the various parts of a dream, originally incoherent and illogical in respect to both time and place, are reassembled and reconstructed into the coherent, logical form in which we afterward recall them. He believes that every dream goes through the process of 'evolution of logical continuity,' and that it is possible to determine this evolution, or at least its general characteristics, from the observation of a large number of cases of dreams with special attention given to the control of the conditions of awaking.

"This law of logical evolution," he says, "will, in favorable cases, enable us to follow up the dream from its organized form to the unorganized form which it must have previous to the time of awaking; to describe the processes by which the change is accomplished; and even going far back into the past to find the original sources of the

dream, the sensations from which it arises, the transformation of the impressions made by these sensations, and the forces which produce and direct the transformations."

This ambitious prediction, or program, is hardly warranted by the evidences set forth in the article.

W. I. MITCHELL.

UNIVERSITY OF IOWA.

CHILDREN'S DRAWINGS.

Ein Beitrag zur Kenntnis der Kinderzeichnungen. DAVID KATZ.
Zeitsch. für Psychol., 1906, XLI., 241-256.

This paper reports an experimental study of children's drawings, chiefly in the interests of the general psychology of perception. The writer holds that previous accounts of children's drawings have been too largely on the merely descriptive level. The children have been asked to draw from memory or imagination, not from perceptual objects.

His subjects, three little girls of five, six, and seven years, of whom all but the youngest had been to school, were told to make drawings (1) of blue paper models of geometrical figures and (2) of properly drawn representations of these models. The models used were triangle, square, parallelogram, ellipse, cross, cube, square with four legs like a table, three-sided pyramid, cylinder, and three-sided prism. The children were given pencil, paper, and ruler, and were directed to reproduce the objects very carefully and to keep the same places so far as possible. The models remained at a fixed distance.

Reproductions are given of the children's drawings of the table, the pyramid, the cube, the cylinder, and the prism, as well as some typical examples of their drawings from the plates of the table, the cube, and the cylinder, with the correct forms accompanying them for comparison.

These drawings reveal the usual childish disregard of perspective. The top of the table is represented as a square with the legs in the same plane running in one case diagonally out from the corners of the square, etc. In the cylinder, both ends show at once; in the cube, all four sides. The drawings of the five-year-old subject show the greatest lack of approximation to the actual appearance of the models. In the attempted reproductions of the correct drawings only those sides of solids are given which were present in the copy. But the youngest child, for instance, drew the three visible surfaces of the cube as three connected squares in the same plane, two above and one below. The

second child drew the top of the table as square, but made some effort to reproduce the vertical position of the legs.

From these drawings Katz confirms the conclusions of Sully and others that the child draws not what he actually sees but what he already knows about the objects before him. To explain the drawings more satisfactorily than has been done, he discusses the growth of the child's knowledge of the object. A developed perception is made up of the idea or meaning of the object and the sensation-complex which stands as the symbol of this meaning. In adult life attention is directed to the significant object, not at all to the mere sensation content. Adult perception means belief in the object.

With the child, however, in his early experiences of, say, a square and a cube, the bare visual and tactual sensation-complexes form the most important element. Originally present as distinct series, they become associated through concomitant changes. Gradually the sensation-complex aroused by the object in a position of maximum stimulation (for instance, a square surface upright in the median plane of the observer) holds attention better than those aroused by the object in other positions, and so becomes the symbol or type of it in any position whatever. This holds equally true for the cube. The child perceives it in terms of his most intense sensational experience, and draws it with four sides as he knows it through tactual exploration. Similarly, though with less uniformity, he draws an object of its actual size rather than the size it appears to have. The facts of color perception show the same thing. We see a color as uniform, in spite of shadows and different kinds of illumination. The color perceptions of children need to be more fully investigated.

Perception chiefly in terms of the immediate sensation-complex, as revealed in a correct drawing, Katz calls adequate perception; perception largely modified by past experience, as revealed by the children's drawings, inadequate perception. The terms seem to the reviewer unfortunate. Since children can give no introspective account of their perceptual experiences, Katz holds that conclusions from their drawings should be supplemented by the reports of those born blind who have regained their sight. His account of perception is practically that of the best modern psychology of the subject. The number of subjects and of drawings in his study is meager, and his conditions apparently were not rigidly controlled. Moreover, the geometrical materials used seem unduly abstract as a basis for a fair test of the normal perceptions of children of the ages given.

ELIZABETH KEMPER ADAMS.

SMITH COLLEGE.

DISCUSSION.

THOUGHT AND THINGS.

Professor Moore's remarks upon my book of the above title in the March BULLETIN lead me to make certain explanations. I shall refer also to criticisms made by others.

The treatment in my book is a compromise between two methods (as suggested by Professor Russell in the *Journ. of Philos.*). I find it impossible to treat each genetic 'mode' in turn exhaustively, by a method that tries to make out longitudinal 'progressions.' For each topic, a certain before-and-after review is necessary, in order to get the movement of consciousness; and this inevitably requires some repetition and restatement of the main characters of the mode in the interest of the particular problem under investigation. So it becomes a question, in order to reach the most effective exposition, of reducing the retracing to the minimum, without at the same time going over to a barren analytic and structural point of view.

To illustrate: in the second volume I find it impossible to discuss the linguistic embodiment of thought genetically without recurring to the general movement of development of thought, which has already been depicted in earlier chapters.¹

As to the use of new terms — on that point, I simply take up any glove the critic may see fit to throw down. It all depends on whether the conceptions worked out are worth naming, and have not been named before (granting of course the relative appropriateness of the terms suggested). In this matter, it hardly behooves me to anticipate the verdict of the public; but the opinion of the *Nation's* reviewer is this: "The vocabulary of well-considered new technical terms that this volume expounds is in itself a precious gift to psychological investigation. For with each of these new terms there goes a valuable new conception." This is by C. S. Peirce, whose opinion is of the highest.

As to the 'static absolute' I don't care for it — and who isn't tired of it? — because it is a purely logical resort, reached as a presupposition of an equally 'static' truth. But as to a 'dynamic relative' — to suggest a contrast term — that sounds just now fresh and very modern; certainly it is less hackneyed than the other. But when we look it in

¹ I cite this instance since the chapter on 'Thought and Language' is to be printed in the PSYCHOLOGICAL REVIEW for May, and may be glanced at by the reader.

the face, what is its complexion? It is a postulate of a *practical dualism*, as *crassly unintelligible* as the other is *logically over-theoretical*. I can't rest content with a 'dynamic' that has nothing outside to move it and no reason inside for moving! If experience proceeds by readjusting to situations, whence comes the situation that 'puts it up' to it to readjust? Why does it grow discontent with its own habit-world? Is a discrete unintelligible dynamic any better than a contentless formal static? To kick where there are no pricks may satisfy a strenuous 'relative,' but beyond endangering our collections of precious antique china, its only result is to strain its own leg-tendons! I'll stake the whole business on Professor Moore's answer to the following two questions:

First, how can experience of the dynamic-relative type secure or utilize knowledge that is socially valid, without at the same time reinstating other things as valid as the social fellows, including the thinker himself?

Second, how can an experience that has no environment save its own habit, and no reality save its present function, get up any 'dynamic' at all?

Or to put these two questions in one: In what sense is the will of the mother spanking the child part of the habit of the child, and why does the child's experience take on this particular phase of 'relative dynamic'?—this occasional and very disconcerting phase of habit?

The 'relative dynamic' is all right in its place; but so is the relative 'static.' To be 'relative,' we must be *dualistic, realistic*, and—'things are what they seem' becomes the motto of a 'radical empirical' pluralism.

My own view, to be argued fully enough in my later volumes, is that the æsthetic is a mode of experience that not only reconciles these dualisms and pluralisms, *content-wise—in a cross-section*—but also continues its mediation *progression-wise, longitudinally*: so that we can fairly say that in it experience has a way of finding its dynamics intelligible as a truthful and so far static meaning, and also of acting upon its established truths as immediate and so far dynamic satisfactions. In short, our relativisms are contrast-meanings, dualisms, instrumentalities one to another, and the mediation and abolishing of these contrasts, dualisms, means to ends, removes the relativities and gives the only tenable 'absolute.' This is the sort of 'absolute' experience is competent to reach. If you ask why this does not develop again into new relativities, I answer, *in fact* it does; but *in meaning* it does not. For the meaning is the universal of all such cases of media-

tion. If the mediation effected in the æsthetic is one of *typical meaning everywhere in the progression of mental 'dynamic,'* then it is just its value that it discounts in advance any new demands for mediation which new dualisms may make. The æsthetic is absolute then in the only sense that the term can mean anything: it is *universal progression-wise, as well as content or relation-wise.* It mediates the *genetic dynamogenies as well as the static dualisms.*

The following brief explanations are relevant to certain of Professor Moore's difficulties.

As to 'meaning,' I hold that after meaning arises as over against mere present content, then the content also of necessity and by contrast also becomes meaning; since then consciousness may intend or mean either, or the difference between the two. As I put it in the book, with the rise of meaning there arise *meanings* (in the plural). To hold a content to just its bare presence is to make it a meaning — after consciousness is once able to *mean 'that only and not anything else.'*

As to the distinction between 'general' and 'schematic,' between 'belief' and 'assumption' — that is the radical position of my entire work, and I am glad to have it called attention to. It connects with and carries further the 'assumption' theory of Meinong and the Austrian school. My second volume rests its interpretations directly upon this distinction. To say that 'schematic' meaning, 'assumption,' is not 'general,' nor 'universal,' nor 'particular,' nor 'singular' — all of which I do say — is to say that it is a mode of meaning *sui generis.* It is the *intent of question,* assumption, hypothesis, prospective reading — over against all the other meanings just mentioned, which are those of *belief,* acceptance, retrospective reading, proof. It means that the instrumental intent of a meaning is *not a general meaning, but goes before it.* A meaning can be instrumental *only to a general not yet reached.* And a 'general' meaning, when made instrumental to further discovery, is then not general, but becomes in turn again 'schematic.'

So far from being antagonistic to an instrumental view, this furnishes the clue to it. On this distinction, *and this alone* — one destined I think to prove the most fruitful in the epistemology of modern times — the logical processes can be construed as essentially experimental from start to finish. This is the attempt of my second volume, which is now in press.

In respect to 'effort' and the 'subject,' I do not resort to any hypothesis of 'activity' in a philosophical sense. I find that, for the 'knower' himself, the sense of effort — whatever its mechanism¹ —

¹ The kinæsthetic theory is most likely, to me.

distributed variously as 'efforts' here and there, gets *segregated in a sense of control* which is the 'self' of judgment.

As to the 'inwardness' of thoughts — I hold that as thoughts they are in a context of reflection, recognized and intended as such; but that there is always that belief-reference which acknowledges or assumes the original control-sphere. I reflect alike on 'serpents' and 'sea-serpents,' but any intelligible use of them as meanings presupposes the reference to their respective existence spheres. This reference is for me the essence of 'truth' as meaning. The serpent idea is 'true' when referred to the proper sphere, and so is the sea-serpent idea. There is always the *matching of experiences* as between *what is true* and what the true *is true to*.

One other point. While saying that the entire world of objects of experience or reflection is such to a self or subject, and is also referred to its original control, which 'holds the entire system to its moorings,' I mean two things besides. First, the original control, the 'moorings,' to which each idea or object of reflection is referred, is itself *the experienced* or made-up set of meanings of that original mode — as the reference of the idea 'horse' to the perceptual horse-experience — the envelope of the developing psychic process *being nowhere ruptured*.¹ The controls, 'foreign' as well as 'inner,' are all psychic meanings. And second, the dualism of controls does, as Mr. Moore suggests, live to the last within the sphere of logical meanings; it will not down; the dualism of reflection itself is a redistribution, *not a mediation*, of the control factors. But my conclusion from this is not a dualistic one, and not one of intellectualism; but one of a-logicism. For the failure of the logical to mediate its own and the earlier dualisms is just the opportunity of a genuine mediating experience. The necessity of logic is the opportunity of æsthetic. It is the cry of embarrassment of logical finality, on the one hand, and of pragmatic relativity, with its cruder dualisms, on the other hand, that has rung down the passages of history, and inspired the various solutions of immediacy, all the way from the logical postulates of pure identity, to the affectivist postulates of mystical contemplation. However ineffective these historical 'immediacies' may have proved, they have recurred and will still recur. My own effort is to find out just what is universal and saving in this recurrent endeavor, seeing that genetic analysis shows the endeavor to be inevitable.

J. MARK BALDWIN.

¹ It is however an envelope of inter-psychic or common, in no sense private meaning, as I argue in detail in the forthcoming Vol. II.

BOOKS RECEIVED FROM MARCH 5 TO APRIL
5, 1907.

- Philosophische Terminologie in psychologisch-sociologischer Ansicht.* F. TÖNNIES. Leipzig, Thomas, 1906. Pp. xvi + 106. M. 3.50. [Reprint of the Welby prize essay, from *Mind*, 1898, with additions.]
- L'art et l'hypnose. Interprétation plastique d'œuvres littéraires et musicales.* E. MAGNIN. Pref. by TH. FLOURNOY. 2d ed. Paris, Alcan, no date. [Fine illustrations by F. Boissonnas of the plastic attitudes under hypnotic suggestion of Mme. Magdeliene G.]
- La morale sexuelle.* A. WYLM. Paris, Alcan, 1907. Pp. iv + 327. Fr. 5.
- Essai critique et théorique sur l'association en psychologie.* PAUL SOLLIER. (Bibl. de philos. contemp.) Paris, Alcan, 1907. Pp. vii + 188.
- The Psychic Riddle.* I. K. FUNK. New York and London, Funk & Wagnalls, 1907. Pp. viii + 243.
- Das Pferd des Herrn vom Osten (der kluge Hans).* O. PFUNGST. Intr. by C. STUMPF. Leipzig, Barth, 1907. Pp. 193. M. 4.50.
- Raum und Zeit.* F. RATZEL. Ed. by P. BARTH. Leipzig, Barth, 1907. Pp. viii + 177. M. 3.60.
- Der Sehraum auf Grund der Erfahrung.* R. VON STERNECK. Leipzig, Barth, 1907. Pp. vii + 96. M. 3.50.
- The Evolution of Matter, Life, and Mind.* W. S. DUNCAN. Philadelphia, Index Co., 1907. Pp. 250.
- Bericht über den II. Kongress für experimentelle Psychologie (Würzburg, April, 1906).* Ed. by F. SCHUMANN. Leipzig, Barth, 1907. Pp. xviii + 266. M. 9.
- The Persistent Problems of Philosophy.* M. W. CALKINS. New York and London, 1907. Pp. xxii + 575. \$2.50 net.
- Famlienforschung und Vererbungslehre.* R. SOMMER. Leipzig, Barth, 1907. Pp. vii + 232. M. 9.
- Psychology Applied to Medicine.* D. W. WELLS. Philadelphia, Davis Co., 1907. Pp. xiv + 141.
- Aspects of Kinetic Evolution.* O. F. COOK. (Proc. of the Washington Academy of Science, Vol. VIII.) Washington, 1907.
- The Political Thought of Plato and Aristotle.* E. BARKER. New York, Putnam; London, Methuen, 1907. Pp. xxii + 578.

The Integrative Action of the Nervous System. C. S. SHERRINGTON. London, Constable; New York, Scribner, 1907.

Stereoscopic Vision and its Relation to Intensity and Quality of Light Sensation. T. R. ROBINSON. (Univ. of Toronto Studies.) Toronto, The University, 1907. Pp. 78.

NOTES AND NEWS.

THE annual meeting of experimental psychologists will be held at the University of Pennsylvania on April 17 and 18.

DR. CHARLES H. JUDD has been promoted to be professor of psychology and director of the psychological laboratory at Yale University, and Dr. William E. Hocking has been made assistant professor of philosophy at the same institution; Dr. Hocking will assume his duties in the fall of 1908.

THE following are taken from the press:

AT Columbia University Dr. W. P. Montague has been made adjunct professor of philosophy, Dr. Harold Chapman Brown has been advanced to the position of tutor in philosophy, and Mr. Walter B. Pitkin has resigned to accept an editorial position.

MR. NOBLE HARTER, who conducted a research on the telegraphic language in conjunction with President W. L. Bryan, of Indiana University, died in Pasadena, Cal., on February 23.

IN view of the death of Professor Garman, Professor F. J. E. Woodbridge, of Columbia University, will lecture before the department of philosophy at Amherst College during the present term.

AT Brown University lectures have been given by Professor Josiah Royce, of Harvard University, on 'Provincialism,' and by Professor E. C. Sanford, of Clark University, on 'The Rôle of the Different Senses in Mental Life.'

THE Alice Freeman Palmer fellowship at Wellesley College has been awarded to Miss Helen B. Cook, who will study psychology in Germany.

THROUGH a recent donation by Mr. Francis Galton, London University has been enabled to extend the scheme for the study of national eugenics founded under his previous benefaction. The work will be carried on under the supervision of Professor Karl Pearson, in consultation with Mr. Galton, with a corps of active assistants.

